| | Dry matter. | Containing nitrogen. |
|---|---|---|
| Without manure . . . . . | 13.5 grams. | 0.23 grams. |
| With nitrogen . . . . . | 15.3 " | 0.26 " |
| With potash and phosphoric acid, | 17.0 " | 0.29 " |
| With potash, phosphoric acid, and nitrogen . . . . . | 24.4 " | 0.41 " |

The nitrogen of the unmanured soil was not sufficient to fully supply the needs of the barley; for while manuring with potash and phosphoric acid only enabled it to produce 26% more dry matter, containing 0.06 of a gram of nitrogen, the addition of 0.2 of a gram of soluble nitrogen enabled it to show an increase of 81% of dry matter, containing 0.18 of a gram of nitrogen.

These facts admit of but one conclusion; viz., that peas are able to assimilate the nitrogen contained in the soil much more readily than is barley. The fact that the pea-plant contains much more nitrogen than the barley-plant does not show that peas should receive much more nitrogenous manure than barley, but, on the contrary, that they can readily supply themselves with nitrogen, but need to be manured with potash, and particularly with phosphoric acid. Barley, on the other hand, contains little nitrogen, partly because it cannot gather it readily, and therefore it needs an artificial supply. In other words, the greater need of nitrogen on the part of the peas corresponds to a greater power of obtaining it.

It is, of course, unsafe to generalize from these two experiments. At the same time, their results correspond so exactly with the teachings of experience regarding the most suitable manuring for legumes and cereals respectively, and appear *a priori* so probable, that one can hardly avoid a strong belief in their general application. They certainly open an interesting and important field for further research. If it can be shown, that, in manuring any given plant, we ought to direct our attention more particularly to those elements of its food which it contains in relatively small quantity rather than to those present in abundance, we shall have made a very considerable advance in our knowledge of the theory of manures.

H. P. ARMSBY.

## *KOCH'S WORK UPON TUBERCULOSIS, AND THE PRESENT CONDITION OF THE QUESTION.*

THE question of the cause of that form of disease known as tuberculosis is one which has been the subject of discussion in medical circles for many years. It is of especial interest to the laity, because in one of its forms it includes the affection so widely known as consumption of the lungs, or phthisis. The idea of a contagious nature as belonging to this process, i.e., to tuberculosis, was first broached in modern times by Villemin,[1] as the result of a series of experiments upon animals, conducted by him. These experiments attracted very great attention at the time, and were subsequently repeated, with varying degrees of success and failure, by numerous observers. Twenty-five years before Villemin's experiments were announced, Klencke [1] claimed to have produced tuberculosis in animals (rabbits) by the inoculation of tuberculous matter. His results do not, however, seem to have received the attention which they deserved; and it is to Villemin that is usually ascribed the beginning of the line of experiment which has resulted in the work which is under consideration to-day.

Among those who have taken up the question of the specific nature of tuberculosis in inoculation experiments, may be especially mentioned Waldenburg, Klebs, Cohnheim, Fränkel, and Baumgarten. Inhalation experiments, in which the disease is sought to be communicated by forcing animals to inhale finely divided dried tuberculous materials, have been tried again and again with as conflicting results as in the preceding series. Those who have done the most noteworthy work in this direction are Schottelius, Tappeiner, Weigert, Weichselbaum, and Balogh.

Feeding-experiments form the third class by which an endeavor to obtain evidence for or against the specific nature of tuberculosis has been made. It is unnecessary to do more than mention the names of a few of those who have taken a prominent part in this branch of the investigation: such are Aufrecht, Klebs, Bollinger, Colin, Tappeiner, and Toussaint.

These names, forming but a small part of the catalogue of those who have been interested in the study of tuberculosis, will give some indication of the vast amount of work done, and the interest taken in this subject.

After Villemin's experiments, and coincident with all the work that was called out by them, the question of the *nature* of the virus of tuberculosis was eagerly discussed. The idea of a *contagium vivum* was first suggested by Buhl,[2] who claimed to have observed micro-organisms constantly occurring in tuberculous nodules; these micro-organisms being both micrococci and bacteria. This idea was taken up by Klebs,[3] who claimed to have isolated a micrococcus by culture, and to have produced tuberculosis by the inoculation of this organism. Klebs's experiments were repeated, and with the same, or nearly the same, successful results, by Schneller,[4] Reinstadler,[5] and Deutschmann.[6] The acceptance of this monas tuberculosum, as it was called, as the specific cause of the tuberculous process, was not general, however; and for various reasons the work of Klebs seems to be untrustworthy.

[1] *Gazette médicale de Paris*, December, 1865. Études sur la tuberculose. Paris, 1868.

[1] Untersuchungen un erfahrungen, etc. Von Professor KLENCKE. Leipzig, 1843. Bd. i.

[2] Lungenentzundung, tuberculose, und schwindsucht, 1873.

[3] *Prager med. wochenschrift*, 1877, Nos. 42 and 43.

[4] *Ueber therapeutische versuche. Arch. für exp. pathol.*, bd. xi., 1879. *Exp. und histolog. untersuchung über die entstehung der tuberculose, etc.*, 1880.

[5] *Arch. für exp. pathol.*, bd. xi., 1879.

[6] *Med. centralblatt*, No. 18, 1881.

While all these investigations were going on, and the contradictory and conflicting results derived from them were being given to the world, other experiments were being conducted, the results of which were not announced as the work progressed, but were kept from publication until they had been verified in the most complete manner that modern methods would permit. These experiments were those of Robert Koch, conducted by him at the laboratory of the German board of health in Berlin, and pursued with unremitting diligence and care for over two years. The results were first made public under the modest title of 'The etiology of tuberculosis,' at a meeting of the Berlin physiological society in March, 1882, and were published in the *Berliner klinische wochenschrift*, 1882, No. 15. His method of work was as follows: —

Starting with the assumption that a micro-organism might be at the bottom of the disease, he carefully searched for some evidence to support this theory by microscopic investigation of large numbers of tuberculous tissues from various sources. As a result, he found, that with favorable illumination of the specimen, and good lenses, it was possible to make out the almost constant presence, in tuberculous tissues, of a rod-like organism much smaller and finer than most of those that had hitherto been observed. The occurrence of this organism was found to be so frequent, and in such early stages of the disease, that a suspicion of its causal relation to the pathological process was forced upon him.

The discovery of the existence of this bacterium was but the beginning of the investigation, however; and the masterly series of experiments by which he went on to prove its specific relationship must be read to be appreciated.

In the first place, it was necessary to isolate this organism from its surroundings, and to propagate it by itself; that is, to produce a 'pure culture.' The best means to do this could only be ascertained by experiment; and, after the conclusion of these experiments, his results were these. The organism was found to flourish best at a temperature of from 36° to 40° C., — a much higher range than is necessary for most forms of bacteria. This being ascertained, it was necessary to find some suitable culture-soil upon which the organism could flourish; for the ordinary gelatine media would not remain solid at this temperature. Here it was found that the serum of the blood of sheep or cattle was the best medium to be employed; for, by exposure to a comparatively low temperature (65° C.), it would gradually solidify, until, at the end of a few hours of such exposure, it would become a transparent, amber-colored mass of a jelly-like consistency, of course remaining solid at a lower temperature for any length of time. After this was done, there was still another peculiarity of this organism to be appreciated, and that was its slow growth. Up to this time, bacteria were supposed to complete the cycle of their existence in a very short time, — usually measured by minutes, occasionally by hours. In this case, however, there was something entirely different; and it was not until after a large number of experiments had failed, that it was realized that they were dealing with an organism requiring from a week to ten days for any appreciable increase in its numbers to occur.

Having found out all these peculiarities, it remained to study what happened in the culture-apparatus, as time went on. It was found that it required from two to three weeks for such a growth to take place upon the surface of the culture-medium; that a portion could be transferred to another nutritive soil, and a new culture started: it was found that the growth occurred in dry, whitish-gray scales, that, under a low power of the microscope, were seen to be rather of a sigmoid form; and it was found that when these scales were transferred to other soil and broken up, each fragment would produce like scales, and that this method of propagation could be kept up for months at a time. Inoculation of animals of various kinds with material from these cultures at any and all stages of their growth, done under the most rigid precautions for the exclusion of any impurities, or possibly specific matter, was found invariably to produce the disease tuberculosis. The tissues of these animals, when killed, presented the nodular appearances peculiar to the disease; and in these nodules were always found organisms exactly similar to those which had been injected. Cultures of these organisms showed exactly the same peculiarities as in the first instance; and inoculations with the result of these cultures produced the same pathological appearances.

In order to prove that these organisms *alone* would produce the disease, Koch used other substances than tuberculous matter for inoculation; and these substances being proven by microscopic examination to contain no organisms, and being protected from external contamination by all known precautions, gave entirely negative results, and in no case was tuberculosis produced. On the other hand, in no case was there a failure to produce the disease, when materials containing the organism, or the organism itself, were employed as the inoculating material.

Thus it will be seen that the conditions necessary for the establishment of a specific causal relationship between a micro-organism and a given disease were fulfilled in this case, so far as it was possible for one observer to bring them about. The constant occurrence of an organism in the varying forms of the disease in animals and in man; its isolation from the tissues, and reproduction by artificial means; its introduction into healthy animals, with the resulting pathological processes exactly similar to the original disease; its discovery in these inoculated animals; and its re-isolation and observation, — all these requirements have been repeatedly fulfilled in the case of the bacillus of tuberculosis, together with that other trying one, that materials proven to contain no bacilli invariably fail to act in any specific manner.

Not content with the work thus announced, Koch went on for a year longer with his experiments, the results of which were collected in July of last year, but have only recently reached American readers

in the second volume of the German health reports.[1] In this article, Koch re-affirms his original announcement, and gives the results of further work in the same direction, all reached by experiments conducted with the same precision as the first series. They bear out his assertions to the full, and with the exception of a few slight changes of technique, and a modification of the staining methods employed, have led him to no change whatever in regard to his views as first expressed more than two years ago.

These two papers taken together form a monument of scientific accuracy and care, and, so far as subsequent investigations go, will carry conviction to the mind of any impartial judge. Confirmatory evidence, as regards the occurrence of the organism in question in tuberculous lesions, has been offered upon all sides, and in enormous mass. The real evidence, however, the repetition of the culture and inoculation experiments, is sadly deficient. This is, perhaps, not to be wondered at, because the apparatus necessary is so extensive, the training so severe, and the aptitude for the work so rare. In addition to all this, the time necessary for the experiments is so great, that the chances are that they never will be repeated to their full extent, although it is only by such thorough and exhaustive investigation that progress in this branch of scientific medicine can be expected.

Some few observers have pretended to upset the conclusions of Koch upon the basis of extremely unsatisfactory and incomplete observations. Spina of Vienna is, or was, a prominent champion of this class. His book was announced with a flourish as being intended to overturn, and as actually accomplishing the destruction of, all Koch's theories. Upon its publication, it was found to be nothing but a criticism of methods that Koch himself acknowledged to be faulty, and a few observations upon the occurrence of the bacillus, but with an entire absence of any culture, or properly conducted inoculation, experiments whatever; in all respects being so far below the work it was meant to criticise, that it was with pain and mortification that we heard it mentioned on the same plane, in the annual address to the Massachusetts medical society of this year. This, however, seems to be the limit of any noteworthy objections in Europe: in this country it is different, a number of gentlemen having considered themselves authorized to speak in opposition to Koch's views upon the ground of personal observations. For the most part, however, their pretensions are too weak to receive serious notice: as, for example, Schmidt's cry of 'fat crystals;' Gregg's, of 'fibrine filaments;' Cutter's, of 'Mycoderma aceti;' or Formad's, of 'narrow lymph-spaces.' It is, perhaps, hardly fair to speak of Formad's deplorable failure to maintain his opposition to Koch by any reasonable arguments at the last annual session of the American medical association. Through imperfect counsel, the gentleman was induced to come before the meeting, and after announcing far and wide his intention to give results that would destroy the last vestige of strength to Koch's assertion in regard to the specific nature of the bacillus of tuberculosis, instead of doing this, proceeded to read a reprint of an article published by him last fall, announcing that his results would be published in the near future. This, in the present condition of all questions relating to micro-organisms in this country, seems to be almost inexcusable. These results have been promised for months, and at the time of writing have not yet appeared. It seems as if it were the bounden duty of all those honestly interested in the advancement of scientific knowledge to talk and publish less, and to work more. What is needed is the publication of the results of work carefully and conscientiously performed, together with the exact details of every step in every process by which those results were reached. In addition to this, we have a right to demand that all work of this kind shall be done by trained observers, in the presence of others equally well qualified for the observation, — not with and by half-trained students, — and that the very best appliances of modern research shall be employed in each and every observation made. In this way, and in this way only, can reliance be placed upon observations recorded in work on micro-organisms; and it is the absence of work of this kind which gives so very little force to the opponents of the specific nature of the bacillus of tuberculosis. At the same time, it is the presence of this very accuracy of the detailed account of every step in the process by which the results were reached, and of the completeness of the experiments and control experiments, that gives the convincing power to Koch's work. Nothing that can be for an instant compared with it for simplicity and directness of statement, or completeness of detail, has yet been brought forward by his opponents. Until that is done, and it does not look probable at the present writing, his work must be accepted as conclusive; and measures should be taken to control to some extent the wide-spread destruction of this disease, as it is most certainly within our power to do.

Koch's own work upon the subject of tuberculosis has been suspended for a year, owing to his absence in the east with the German cholera commission, with which he has lately returned. Whether he himself will take it up again is to be doubted; for his facilities for work are unbounded, and his natural impulse will, of course, be to open up untrodden paths of research.

[1] *Mittheilungen aus dem rais. gesundheit*, bd. ii., 1884.

## *THE GREENWICH OBSERVATORY.*

THE board of visitors of this institution held its annual session on Saturday, June 14, and heard the report of the astronomer royal on the work of the observatory during the twelve-month ended May 20. Of this, Mr. Christie says, "It has gone on steadily in the same lines as in former years, with such small extensions in certain directions as could be made without infringing the long-established principle that